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THE CONTRAST BETWEEN SCIENTIFIC THEORY AND THE DEMANDS OF THE PRAGMATIC PRESCRIPTION¹

IN an earlier paper² it was pointed out that corresponding to any fact or law there is definable the set of conditions without which the fact could not be, or without which the law could not be even possibly true (much less probably true). It was shown that the opinion embodying these conditions is frequently known in science as a *theory*. The name, *theory*, was suggested for all such opinions. The theory is as true as the fact. The opinion answering to the conditions so defined would necessarily be workable in an extraordinary degree. Unless this opinion were the true one, things could not be observed to be as they are. The opinion then certainly is one which *works* for the observed facts; and it is this aspect of the matter—and to my mind it is the least important aspect, and certainly not the one which gives theory its claim to be true—that is universally emphasized in the textbooks of science.

The undulatory theory of light works better than the corpuscular hypothesis; it explains things which the discarded opinion could not explain. The kinetic theory of gases works so well that one can predict, with its help, laws which are actually verified. An important textbook of physical chemistry says:³ “A theory, to be accepted as useful, must enable us to deduce known facts and new facts. The kinetic theory of gases certainly fulfils the requirement, for we can deduce from it the gas laws and Avogadro’s hypothesis.” The theory here, it seems, should be *accepted as useful*. It is wholly overlooked that the kinetic theory, in the main (in the main, for I do not think that every bit of the kinetic theory answers the requirement) is a statement of the conditions without which a large group of phenomena could not be. As such it is true. Now, I say, it is *not* remarkable that true premises should enable one to predict verifiable conclusions. For the true implies only the true. Logicians know, however, that the false also implies the true; and it may well happen that an hypothesis, though false, still leads to verifiable predictions. Thus an hypothesis may be false though workable. And the scientist who lauds a theory, because it leads to verifiable predictions, has been offered bread, but has chosen a stone. He has turned his back upon the most valuable attributes of his theory. The true is always workable, but the workable is not always true.

The inductive process whereby an hypothesis after repeated veri-

¹ This paper was read at Professor Royce’s seminary, January 18, 1916.

² See this JOURNAL, Vol. XIII., p. 236.

³ S. L. Bigelow, *Theoretical and Physical Chemistry*, 1914, p. 140.

fications becomes a law is an evolutionary process in the advancement of learning. The opinion which is possibly true rises to the plane where it is probably true. Here is improvement—and growth of knowledge. But, for an hypothesis to become a theory—speaking strictly, there is no such thing. The hypothesis, however workable it may be, is or is not already a theory. Some one notices that these are the conditions without which the phenomena could not be, and—lo, we see that what we had taken all along for an hypothesis is really a theory after all—and it's true. What is more natural than that it should have led to verifiable predictions. The multiplication of instances of successful prediction demonstrates more and more the workable character of the hypothesis, but it proves nothing; and if it happens that it is a theory already that we are considering, our ability to predict from it facts that we already know adds not one jot to our knowledge.

I. SOME PRAGMATIC AND OTHER ASPECTS OF THE KINETIC THEORY OF GASES

In 1738 Daniel Bernouilli came forward with the hypothesis that gases consist of particles moving about with great rapidity, hitting against one another and against the walls of the containing vessel, bouncing off again, like elastic balls, to continue their movement, with unimpaired velocity, in a new direction. According to this hypothesis the pressure of the gas would be due to the many impacts of the many particles upon the walls of the containing vessel; and, consequently, the pressure might be increased either by increasing the number of particles in the given volume or by increasing the velocity with which they moved.

More than a century later, in 1851, Joule showed that one of the consequences of this hypothesis is Boyle's law—and that law is known to be genuine. The successful prediction from this hypothesis of verifiable consequences brought it again to the attention of scientists. It was taken up by Krönig and Clausius in 1857 and has since been developed by many others in its various ramifications.

Joule's reasoning was essentially as follows: Suppose that the quantity of gas contained in a cubical container, whose sides are l centimeters long, consists of n particles, each of the mass m , and each moving with an average velocity V . From a consideration of the parallelogram of forces, it follows that the force of impact of the particles striking against any side of the cube is the same as it would be if one third of all the particles ($\frac{1}{3}n$) were moving in a direction perpendicular to that side. One particle will require l/V seconds to move from one side to the opposite one, and will therefore strike the given side $\frac{1}{2}(V/l)$ times per second. The momentum

of the moving particle is mV and on bouncing away from the side after the impact it has a momentum quantitatively the same, but of opposite sign. Its change of momentum then is $2mV$. Thus the total change of momentum of $\frac{1}{2}n$ particles striking the side $\frac{1}{2}(V/l)$ times per second will be $[\frac{1}{2}(V/l)](2mV)(\frac{1}{2}n) = \frac{1}{2}(nmV^2/l)$. This is the total pressure exerted on any side. Since the area of the side is l^2 , the pressure per unit area will equal $\frac{1}{2}(nmV^2/l^3)$. But l^3 equals the volume v of the cube. Therefore $p = \frac{1}{2}(nmV^2/v)$ or $pv = \frac{1}{2}nmV^2$. For the gas under consideration n , m , and V are constant values; then the total of everything on the right of the equality sign is constant, and $pv = K$ —which is the expression of Boyle's law. The ingeniousness of the reasoning demands admiration.

Joule's reasoning is brought forward very frequently as evidence in favor of the kinetic theory. Aside from the mathematical part it amounts to this:

1. If the kinetic theory were true, Boyle's law would be a physical necessity.

2. Boyle's law is genuine (instances of it are fact). Would any one think of concluding from these propositions, "Therefore the kinetic theory is true?" Stripped of all concrete significance, the propositions become

1. A implies B ,

2. B is true;

and it certainly does *not* follow that A is true. The true implies the true, the false implies the true, and the true may be implied by either the true or the false. No judgment upon the truth or falsity of A is justified. Yet A works.

My only excuse for discussing these truisms of logic is their bearing upon pragmatism. The kinetic theory as used by Joule *worked*; its pragmatic value for him could be defined as its pointing toward verifiable conclusions; its *working* lay in the fact that it implied the true. Yet we know that the true may be implied by the false. Science, therefore, must refuse to accept the pragmatic criterion. *Why should we satisfy ourselves with hypotheses which imply the observable, when we can get theories which the observable implies?*

Does the fact that A implies B , while B is true, render A *probably* true? One would be tempted at first sight to say that it does not. Would the fact that A implies B , and C , and D , and E , etc., where B , C , D , and E , etc., are true, render A probably true? One begins to become suspicious. The fact pointed out by Joule, that the kinetic theory implies Boyle's law, is one instance of the workable character of the kinetic theory. It happens moreover that a

great many gas laws and observable properties of gases may be predicted from the kinetic theory. These are further instances of its workableness—and altogether there is quite an array of “fair samples.” The induction is justified that the kinetic theory is probably always workable. If *A* implies *B* and *B* is true, then in this case *A* works. Suppose that in other cases where *A* implies *C*, *D*, *E*, *F*, and so on (where *C*, *D*, *E*, and *F* are “fair samples”) it is still found to work, then it is probable that *A* universally works. That is, it is probable that *A* always implies the true. But that which *always* implies the true is itself true. Then it is probable that *A* (always implies the true and) is true.

In a word, then, “fair sample” cases in which the results predicted from an hypothesis are verified render the hypothesis probably true.⁴ Still it is going rather far to hold up the kinetic theory as probable on the strength of the single successful prediction of Joule. Yet there are so many similar cases that it is indeed highly probable.

The kinetic theory certainly provides an excellent working explanation of a large group of phenomena. But isn't it really more than a plausible hypothesis? Isn't it a theory, in that it is, in the main, a statement of the conditions without which the phenomena could not be? Isn't it true?

There seems little reason to doubt that Joule looked upon his own explanation of the mechanics of gas-pressure as an hypothesis adapted to the fact. Yet at the time in which Joule lived there were sufficient experimental data available to justify his conclusion as theoretically true. Facts which had already been observed implied his explanation. The physical and chemical behavior of gases implied Avogadro's Theory, namely, that equal volumes of gases under the same conditions of temperature and pressure contain equal numbers of molecules (or what may be called “physical units”). The fact of the existence of substances of the same composition, yet palpably different, implied that the matter of which they were made consisted of separable particles. Then certain gases, specifically those which can be used in synthesizing isomeric substances, definitely do consist of particles, and equal volumes contain under the

⁴ This seems a great advance over such inductions as “all swans are white,” “all men are mortal.” For, here, by the examination of one set of things we confer probability on another set. This is a theorem of what might be called “the calculus of fair samples.” All the theorems of this calculus would not lead to probabilities. Another was developed in the paper referred to above. Many others are hidden, I imagine, within masses of scientific data. Such a calculus, if complete, would be the complement of the algebra of logic—for within the two our manner of knowing all things, except the metalogical, would be comprehended.

same conditions equal numbers of molecules, and the molecules themselves contain each a definite number of atoms. Joule knew that gases diffuse into one another to form homogeneous mixtures, that a heavy gas diffuses upward into a lighter one, and that, when the diffusion is complete, there is never any tendency on the part of the heavier gas to settle out again. Had he reasoned that these things could not be unless the particles of which gases consist are in constant movement, hitting against one another and bounding off again with unimpaired velocity, his conclusions would have had the truth which attaches to a theory. Graham's law states that the rates at which gases diffuse are inversely proportional to the square roots of their densities. This means, since equal volumes contain an equal number of particles, that the rates of diffusion are inversely proportional to the square roots of the masses of the particles. Had Joule reasoned that this law could not be unless the velocities of the particles of gases (at any temperature) were inversely proportional to the square roots of the masses of the particles, his conclusions, again, would have had the truth of a theory. Since the kinetic energy of a moving mass is proportional to the mass and to the square of the velocity (proportional to MV^2), Joule might well have reasoned that all these facts could not be unless the average kinetic energy of a particle of one gas, at a given temperature, were equal to the kinetic energy of any particle of any gas, at the same temperature. All these facts were well known in 1851, and Joule could have demonstrated to anyone who had the patience to listen—and patience is needed, I fear—that the kinetic theory of gases is the necessary implication of the observed behavior of gases. As such it is true.

In his paper of 1851 Joule actually calculated the average velocity⁵ of the particles of certain gases at a definite temperature. It is interesting to notice that the velocities calculated by Joule are actually *true* velocities. Their numerical exactness is limited only by the precision of the physical measurement of the quantities involved in the mathematical calculation.

The viscosity of gases, at any given temperature, is independent of the pressure upon them. If gases are homogeneous throughout, the effect of an increase of pressure would be to increase the quantity of stuff in a given volume, and consequently to increase the viscosity. If gases consist of moving particles, the effect of an increase of pressure would be to increase the number of particles in a given

⁵ In the equation given above p and v can be measured directly; n and m can not be measured, but their product, nm , the number of the particles times the mass of the average individual particle, is equal to the total mass of the gas under consideration. This can be measured, and V calculated.

volume, but the velocity of the particles would not be altered, and the viscosity would remain unchanged. We may say that unless the kinetic theory were true, in the main, the effect of pressure on viscosity could not be as it is observed. Hence the kinetic theory is true. Lord Rayleigh says that the viscosity of gases is, to his mind, the best evidence that there is for the kinetic theory. Professor Richards, who reports the conversation, remarks that this is interesting in that it shows the point of view of the physicist, but for the chemist there are also many other considerations which point toward the kinetic theory. Some of these considerations have been indicated.

The evidence which has thus far been presented for the truth of the kinetic theory is, to say the least, complex and technical. A simple and beautiful experiment has been performed very recently by Dr. Wood, of Baltimore, so simple that its implications would be evident to children in grammar school—and to this experiment the main tenets of the kinetic theory stand in the position of the *conditiones sine qua non*. The confessed object of the experiment was to obtain a gas in which all the molecules were moving in one direction. This result was to be attained by removing all the molecules which happened to be moving in other directions. Mercury vapor was admitted at the end of an evacuated cylindrical glass vessel, the vessel being submerged in liquid air. Since the particles of the incoming vapor were moving about in all directions and colliding with one another, many of them found their way at last to the sides of the cylinder. When they struck the sides of the cylinder, the cold liquid air removed most of their heat, they did not bounce off again, but remained condensed upon the cold glass surface. The rest of the particles, then, were moving in more or less straight lines through the length of the cylinder. At the center of the other end of the cylinder there was a small hole leading into a spherical glass chamber. The mercury condensed in a droplet *opposite this hole*, but nowhere else upon the walls of the spherical chamber. It seems to me that much genius is needed in the man who would devise a single simple experiment which means as much as this one. For any one can see that this experiment *could not be* unless the main tenets of the kinetic theory were true.

The kinetic theory is the rock upon which much of thermodynamics, and of physical chemistry, and of physics is built. That this theory, in the main, is as true as any bit of experience is a highly important matter. It is for that reason that I have selected it for the present discussion—in spite of the fact that the data upon which it is based are not of the most familiar kind. It has appeared that it possesses a truth wholly beyond that inherent in its prag-

matic workings. So it is with all real theories. Theory and hypothesis are wholly unlike. The logical nature of this contrast will appear more distinctly from a consideration of simpler cases. This consideration will serve also to show again that the tendency of scientific men to theorize is not devoted wholly to the production of highly important theories which form the basis for several sciences at once, but that it leads to little pieces of new information (really theories, but seldom called so)—tucked away oftentimes in the corner of some special science, but having that which far more important hypotheses do not have—namely, truth.

II. OTHER INSTANCES OF THEORY

Although sun-spots had been observed with the naked eye before the time of Galileo, his telescope permitted him to make more extended observations upon them. With the new information at hand, he straightway began to theorize. The observation that the spots appeared to move across the face of the sun, coming to him upon the heels of his discovery of the satellites of Saturn, led him to suppose at first that their appearance was due to planets revolving near to the sun and appearing as black spots against its luminous disc. Presently he noticed that occasionally one large spot divided itself into several, and occasionally several combined to form one. The spots varied in degree of darkness. As they approached the edge of the sun's disc they appeared to contract in breadth, without losing in length. Finally, it sometimes happened that a number of spots of varied size appeared in a certain order, and then, after a period of disappearance, they reappeared again in the same order. Galileo gave the true explanation of these phenomena: the spots were not planets intercepting the sun's light, they were on the sun itself and the sun was rotating about an axis. At this distance it is evident that the explanation of Galileo is a *theory*. Unless the sun rotates about an axis, and unless the spots are on the sun, these things could not be as they are observed. Here is no mere working hypothesis. *This* explanation is implied by the observations.

The situation becomes still more interesting when there are several hypotheses, all equally workable for the phenomena under consideration. Sooner or later there comes a time when an observation or critical experiment is made which renders one or more of the hypotheses, maybe all but one of them, now unworkable. The surviving hypothesis still "applies"—subject always to the possibility that a further situation may arise which will render its abandonment or revision necessary. As a matter of logic it *may happen*, as a matter of fact it often *does happen*, that the new observation is such that it not merely renders some of the hypotheses unworkable,

but actually implies or presupposes some other. That which is so implied is, of course, as true as the observation which implies it. It is no guess. It is a theory.

The theory of the manner in which coral islands are formed is an admirable case for the present discussion. There are three kinds of coral islands: "Fringing reefs," which lie along the shore of a mountain which rises out of the sea, the commoner type of coral reef enclosing in its more or less circular form the mountain-island, separated from it on all sides by a lagoon of placid water, and finally, the "atol," more or less circular and enclosing no island. It is known that corals grow only in water of less than a certain depth. Therefore, one may say—however these reefs were formed,—they certainly had their beginnings in shallow water.⁶ Darwin offered the explanation that the coral growth began as a fringe upon the shore of the mountain-island; slowly, through geologic ages, the island subsided beneath the ocean, and new growths of coral arose upon the older coral formation now deeper below the surface. Thus, after the mountain had subsided for a certain distance, the result would be a reef encircling the island and separated from it by a considerable space of water. If the island sank entirely below sea-level, the result would be an atol. This explanation worked. It explained the general circular form of the coral islands; it fitted with the fact that corals do not grow in deep water.

There were other explanations. One supposed that the action of the waves washed the island away, either entirely, or all but the central part, and that the corals grew up afterwards upon the edge of the resulting shoal area. Another supposed that the corals grew first upon the coast of the island, and that the island was afterwards washed away wholly or in part by the action of rains or of the sea. It is to be noticed that these hypotheses also explained how the three kinds of coral islands might have been formed.

Some years later the observation was made in the light of which one of these explanations is seen to be a theory. A geologist, having climbed one of the volcanic mountain-islands of the Hawaiian group (the island was surrounded by a coral reef) noticed that the sides of the mountain were heavily furrowed. The rain-water running

⁶ Throughout this part of the discussion there are two assumptions: (1) That time was as it is now, that temporal processes extend backward farther than we can grope or conjecture; and (2) that the laws of nature (whatever they may happen to be) were effective in this past time even as they are now. I shall not attempt here to defend these assumptions because they are not assumed for the discussion of *theory*, but rather for the discussion of something geological—without them there could be no question of geology. To deny these assumptions is to deny the right of geology to any truth whatever. Grant them, and geology may have its theories just as the other sciences have theirs.

down the mountain had worn valleys in the sides. Wherever one of these met the sea there was a bay. Now the rain-water, in coursing down the mountain, had carried with it large quantities of solid material, else it could not have worn away these valleys. If the mountain-level or the sea-level were not changing, then this material, which was poured out into the water near the shore, would have deposited and formed a delta. Actually there were no deltas; observation showed just the opposite thing, namely, bays. There could be no bays where the valleys met the sea *unless* the island were sinking below the sea-level at a rate faster than deltas could be deposited: therefore the island was sinking. Such a statement is logically of the nature of a theory.⁷

Let us reconsider the whole matter, while the various considerations are stripped of their concrete significance. We have here three hypotheses, H_1 , the Darwinian explanation, H_2 , and H_3 , the other explanations which have been mentioned. All of these work for certain phenomena, A_1 , A_2 , and A_3 , namely, the formation of the three types of coral island. They *imply* these three types, and the three types are observed. That is,

$$\begin{aligned} H_1, H_2, H_3 &\text{ imply } A_1, A_2, A_3; \\ \text{and } A_1, A_2, A_3 &\text{ are observed.} \end{aligned}$$

The observation of Dana, upon the rain-valleys of the mountain-side and the bays upon the shore, A_4 , was an observation of phenomena which are implied by H_1 , but not by H_2 and H_3 (for, if H_2 or H_3 were true, there would be deltas). H_2 and H_3 then drop out of the running. They no longer work. There is left H_1 , the sole-

⁷ Whether King Arthur's sword sank beneath the waters of the lake or the waters rose above King Arthur's sword is a verbal question. Here too, in discussing the manner of formation of coral islands, we may, if we wish, disregard the question of whether the mountain absolutely sinks or the sea absolutely rises. For geology, of course, this is an important question, but it does not bear directly upon the mode of formation of coral islands. This question, too, perhaps, is, or will be, capable of a theoretic answer.

The original hypothesis as brought forward by Darwin insisted upon the *absolute* subsidence of the mountain. The American, Dana, made the observation of the embayments at the foot of the valleys, but he did not appreciate its significance. The *relative subsidence theory* is recent. For the facts in this part of the discussion I am indebted to Professor W. M. Davis, of Harvard.

Some of the theories of science are rigidly implied by the facts. Some are implied only if certain laws of nature (really probable) are assumed to be true. In many cases hypotheses which work by implying the verifiable imply the verifiable only by virtue of certain laws which are assumed to be true. So, whether the implication be rigid or be of this inductive kind, the distinction between theory and hypothesis is still genuine; and, though several of the implications in the foregoing are of this latter kind, no harm is done, so long as the confession is made.

surviving successful hypothesis. It implies all of the relevant facts which have been observed so far. That is

H_1 implies A_1, A_2, A_3, A_4 ;
and A_1, A_2, A_3, A_4 are observed.

A_4 appears here as a crucial observation. It decides against certain hypotheses in favor of some other. That, it seems to me, is as far as pragmatism can get. But it is not yet a complete statement of the logic of the case. The fact that there is a bay where the rain-washed valleys of the mountain meet the sea implies that the mountain is slowly subsiding below the sea. . . .

A_4 implies H_1 ;
and A_4 is observed.

Clearly then, if A_4 is true, H_1 is true also. The true implies only the true. We are as sure of H_1 as we are of A_4 . H_1 is as true as any bit of experience. It is a theory.

The more cases there are in which an hypothesis works, provided the cases are always "fair samples," the more *probable* it becomes that the hypothesis is true. That is the best we can ever hope for from any process which consists wholly of guessing and verifying. But guessing is not theorizing: and for the scientist, the great value of the "method of multiple working hypotheses" lies in the fact that, the more hypotheses he has, the more likely it becomes that one among them will some day be implied by some new experiment or observation. The one so implied is a theory—and it is as true as the corresponding piece of experience. There seems to be many such theories in science.

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THE WILL TO LIVE

ONE of the important facts that psychology contributes to human wisdom is this—that any stimulus, even an unpleasant one, is usually attractive when it is not too strong. This fact is generally dropped into a footnote, as though it were an amusing though negligible accident, but it is quite generally agreed to. I have noticed it recently in works so wide apart as Washburn's *The Animal Mind* (p. 43) and McDougall's *Social Psychology* (p. 150). Washburn says, speaking of Jennings's experiments with the amoeba:

"The significance of the positive reaction is harder to determine. It seems to be given in response not to a special kind of stimulus,